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Physics

DETERMINISM AND PROBABILITY IN CLASSICAL AND QUANTUM PHYSICS

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The Catholic University of America

Reverend Fathers and gentlemen, it is a great pleasure and honor to talk here. I am sure that I am not going to tell you anything new; perhaps the arrangement will be a little different from usual. I would like to arrange my talk in the following manner—first I will talk about the kind of determinism to which classical physics has led; secondly I will discuss the question of the meaning of probability and chance in this scheme which seems so deterministic. This will lead us to the question of what the logical basis of the calculus of probability really is. This is a question which is quite disputed, and I can here only state my own opinion. Finally, I shall discuss how far that first deterministic picture has been overthrown.

Of course, the basis for these classical ideas I will discuss goes back to Newton, but Newton perhaps did not draw the conclusions as sharply as was done in the latter part of the eighteenth century, because the mathematical instruments which he introduced and their consequencies were not developed. What I would like to do is to point out in the first part of my talk those features of the classical ideas which make for this determinism.

Now, as you all know, Newton's law says that mass times acceleration is force. Let us analyze these concepts a little. Let me first consider the following situation. Assume I have two quantities, one of which is the position of a particle, and the other is the time. Then a point P_0 with coordinates s_0 and t_0 will signify that at the particular time to the particle is at the particular place s_0. Now the essence of classical physics, of which Newton's equation is an example, is the following. (I am going for a moment to make matters a bit simpler than they really are.) The essential statement is this, that if I know where I am now, then the physical laws tell me where I am going to

(The above address was delivered by Dr. Herzfeld at Weston College on March 20, 1954.)

be next, i.e., if I know the conditions, then the fact that at this moment I am here determines where the next step is going to be. The procedure is repeated; I am now after this short step at a different place, which will modify the situation, but the fact that I have now reached a certain place completely determines where my next step is going to be. If I have this kind of description, then you see that as soon as I have the initial situation, I can construct step by step and unambiguously the history which follows.

Now what I have done essentially is integrate a differential equation. It is the simplest kind of differential equation, a first order differential equation. It is such that it gives me a rule to calculate from the present momentary situation what is going to follow next.

You see from what I have just done that normally nothing special happens. The process is unambiguous. I am at liberty to choose the point from which I start, but from then on things are fixed.

I have said that I simplified this description as compared to Newton's laws. If the law were such that the physical statement was about the velocity, not about the acceleration, then I would have the situation described above. Actually Newton's law is about the acceleration, which means that it is what mathematicians call a second order differential equations; namely, it tells me that I can choose the point from which I start, and I can choose the velocity with which I first want to go, but from then on the equation tells me what I have to do next.

There are a number of interesting things involved in this, the first of which is that it is a characteristic of my elementary description of the physical situation that it is given by a differential equation and not by some other kind of equation. There is another point involved which I have assumed here, and which is implied but not specified in the statement of Newton. We say mass times acceleration is force. Now the use I have made of that here implies that I take something for granted about the force; namely, that the force is defined numerically provided I know where I am. Possibly I can make it somewhat more complicated, but that doesn't change the matter essentially. The force might vary with the place; the force might vary with the velocity, e.g., if I have a friction, but what I have in mind in the simplest situation is a force like that of a compressed spring, where if I know what spring I have and how it is compressed I know what the force is going to be. This is the kind of assumption which prevails in classical physics. In mechanics the statement "Mass times acceleration equals force" implies (a) it is a differential equation, and (b) I assume that when I say force I know (where "know" means I can give a number to) the force, provided I know where I am, or to be a little more general, what my present situation is. Hence the whole

underlying physical structure is such that if I know the present situation I can calculate step by step what follows next

If I analyze this closer, this implies that I can isolate my system, because I cannot otherwise well define what the force is going to be in the sense in which I have it here. If the force on this piece of chalk depends on where someone in Australia happens to be standing, it is clear that while such a kind of physics is imaginable, it is not practical. The idea is implied, therefore, that I can isolate the system so that my force is going to be numerically given if I know the present situation.

Now there are two points involved in that which should be added to the description of the structure. First I do not have to know anything about the previous history. I assume that all I have to know for the purpose of predicting what is going to follow is the present situation, no matter how this present situation came about. This experimentally is not always true. These few cases where that does not seem to be true are usually given the name hysteresis You have that in ferro-magnetic materials; you have it in what are called elastic after-effects. These are relatively few phenomena and, of course, if you are confronted with such a situation, if you find something which seems to be an exception to a general law, which you have found very widely followed, then you have usually a choice between two courses. You can either say the general law is wrong in all its generality, or you can introduce hidden parameters which save the general law. This is in many cases a question of choice. Actually the latter is what people did in the introduction of the law of conservation of energy. This law was first introduced for pure mechanics. Now you know that when you have friction the law of conservation of mechanical energy does not hold. Therefore you have a choice; if you have friction you can either say that the law of conservation of energy does not hold and give up, or you can say, "No, the law is true for a more general system, but I must then introduce another kind of energy, and only if I take both together will the law hold." In practice that is what people generally prefer, and hope that these hidden things will some day be the subject of direct observation. You will expect, therefore, in the case of hysteresis (where what happens next seems to depend not only on the momentary situation as you can observe it now but also on the previous history) that actually when you say that it isn't completely determined by the present situation, you can hope that you haven't really described the present situation completely.

Now if you want to describe, purely from the formal mathematical standpoint, the processes where history seems to come in directly, like hysteresis, you cannot do so by a differential equation.

What you use then is something called a differential integral equation. In all descriptions where you want to describe after effects or hysteresis you have to use this different mathematical tool. Omitting these few cases, we find that given the starting point the physical law is such that it allows us step by step to follow through the future behavior of the system. In a certain sense one could say that what is involved here is a denial of action at a distance in time. The use of a differential equation means essentially that only neighboring moments of time act upon each other.

There is a further interesting point which I would like to mention in that respect. Assume I knew mathematics, I knew what the consequences of calculus are, but I didn't know Newton. Could I from entirely every-day observation find out whether the fundamental law is about velocity or is about acceleration? To give that answer let me assume that the force is as simple as I can make it, that it depends only on position or perhaps not even on position. Then the answer is very simple. If my physical law is a statement about velocity, then I can throw a stone from where I want but not to where I want, because once my starting point is given, the rest of the path is given; I have no choice and only if my original statement is about acceleration do I have an additional choice—the choice of the starting velocity, which will determine the point to which I can throw a stone. In mathematical terms a first order differential equation has only one integration constant, which I can choose as the point from which I throw, but then there is no further choice. A second order differential equation has two integration constants; therefore I can choose from where and to where I want to go. So if I accept the general structure of classical physics, in the sense of differential equations, in the sense that I assume that the force depends only on the position, or perhaps not even on that, as is the case with practical gravity, then that simple fact that I can aim a stone tells me that the physical law cannot be about the velocity but must be about the acceleration or something more complicated.

There is here a point which in my opinion has not been touched on sufficiently, namely, why does this process fail when I apply it to a living organism. And I would just like to say that if you hear occasionally some physicists state that it is clear there is no free will, they are not really talking about free will. What is implicit in their minds is this: every psychological process is accompanied by an external process in the brain, and these processes are in one-to-correspondence. (It is not claimed that they are causally connected.) If all external processes in the brain are determined by a differential equation or something like that, how can you have free will? If the parallel brain

process is completely determined, how can the soul act freely? This is what these people mean in their denial of free will.

There is one more statement of that deterministic viewpoint which I shall mention now, because I will use it later. This statement is often quoted in its most extreme form as expressed by Laplace. (In this form the existence of living matter is, of course, ignored.) He said that if someone who knew all the laws of physics could know at a given moment the precise location and velocity of all particles which make up the world, it would be possible to predict the future exactly for all times. This is the extreme expression of that classical determinism which constitutes the first topic of our discussion.

The question which now arises is this: what place in that scheme which seems so completely deterministic has the calculus of probabilities? If you believe in the statement of Laplace, what purpose is there in using probability at all—what, in fact, is the meaning of the word chance or probability? Now the first point involved is this: in the classical calculation you are asking a large number of extremely detailed questions, in the sense that if you could perform that calculation of Laplace, then it would enable you to tell at any moment where any single individual particle would be. However, you might as well be satisfied to ask fewer questions, for reasons as simple as the fact that it is just too complicated to ask the detailed questions. And this is the sense, actually, of the classical use of probability. You have a problem which is too complicated, and therefore you decide to ask fewer questions. The calculus of probability was introduced at the close of the eighteenth century mainly for the consideration of games of chance. Let me therefore discuss it from that standpoint.

You apply the considerations of the calculus of probabilities to processes in which certain things, positions or velocities, are repeated. Then, instead of asking exactly what is the sequence of events, you ask yourself how often a certain event occurs. Let me illustrate by the following situation. Assume that what I am considering is rolling dice. In order to exclude the interaction of human beings I assume I have an automatic machine. It is a kind of beaker which is shaken mechanically, with a claw which picks up the die and puts it into the beaker; the machine then shakes and throws. You can easily imagine such an apparatus, which is entirely mechanical and still has all the properties of a game of chance. Now let us look at it a little more closely, to see how we may characterize it. You find that its characteristic property is the following. Assume that the number one is on top and the question is,—"What number will follow at the next throw?" Then the motion of the die in the shaking beaker is entirely determined, but the particular orbit it actually takes will depend on where you start.

If you could put your die in exactly the same position in the beaker every time, then at every throw of the beaker the same number, six, say, would come out every time; if, however, you started out a little differently, then the mechanical motion of the die in the beaker will be sufficiently different so that something else might come out. In this motion there are two things involved. First of all, some role is played by the fact that the die has relatively sharp edges, so that there is a near discontinuity in the motion, i.e., the future motion may vary quite considerably if you change the initial conditions only slightly. That is one characteristic of the motion. The other is that no matter how you start, that will not affect the relative frequency with which each side comes up, provided you do it long enough, but it will affect the sequence.

Now when you apply the calculus of probabilities to a physical phenomenon in classical physics, this is always characteristic of the type of motion that you have—the motion is such that the particular sequence depends, of course, on the initial conditions, and usually it is such that there are near discontinuities present. On the other hand the motion is also such that what mathematicians call the measure (the percentage of time which the different events, like the die coming up six, occur) does not depend on where you start, for the motion is essentially of that kind that will repeat itself in the long run. However, where you start essentially affects where you are in the chain of events, and that profoundly affects the sequence; hence if you wanted to know the sequence you would have to go much more accurately into the problem of initial conditions.

So the situation, then, is this: when I talk about probability in classical physics the probability simply means that I have refrained from asking too detailed questions. I have refrained from taking the order or the sequence in which things occur, and am content with a statement about the frequency with which a thing occurs.

Next, if I talk about chance at all, the chance lies in the fact that I can start without knowing which particular initial position I have picked, since that will affect the sequence but will not affect the frequency with which an event will occur.

As far as I can see, therefore, the only basis of the calculus of probabilities lies in these three statements. (a) I refrain from asking about the sequence and only ask about the frequency; (b) secondly, the phenomenon observed is such that the frequency does not depend very much on the initial conditions, even though the sequence does depend very much on them, and therefore (c) as long as I do not know the initial conditions exactly, I must limit myself to a statement about the frequency, which I can make, whereas I cannot

make a statement about the sequence, since it would involve knowing the initial conditions exactly, an extremely complicated job, if at all possible. I do want to point out strongly, however, that there is no contradiction between the classical assumption of determinism in physics and the use of the calculus of probabilities. How far this statement should be modified when you include living beings is again a different story; I explicitly excluded that.

The strange thing is how this was completely upset by the advent of the quantum theory. This point is quite amusing from the standpoint of logic. You make a statement, "If I can do A, then I can do B." Then someone comes along and says, "Yes, but you cannot do A." We have seen that statement of Laplace that if at a given moment we could know the positions and velocities of all particles, then by knowing the laws of physics we could predict for all the future the behavior of the material universe. Now quantum theory, originated in 1925, says that the "if" condition is, by its very nature, impossible. When I say this, there is a distinction we must make—we must distinguish two types of impossibility. Some things are impossible simply because of the present state of our knowledge or of our technology. We cannot fly to the moon at the moment but we do not see any reason which makes that contradict any law of physics. On the other hand, we think that it does contradict the laws of physics to get a *perpetuum mobile* of the first kind. Now you might have said before the advent of the quantum theory that the fulfillment of the condition contained in Laplace's statement was impossible, but only technically impossible; in other words, there was no reason why it could not be imagined to be fulfilled, and indeed, actually fulfilled better and better. Now what quantum theory says is that there is a natural limit to the things which you can know at the same time, e.g., the velocity and the position of a particle. This limit is given in nature, and you would violate a fundamental law of physics if you could contradict that. If we could ever know exactly both the position and the velocity of any particle, at one and the same time, then the laws of physics as we state them today would be wrong. They would require modification, and no small modification would suffice; something entirely new would have to take their place.

In the above there is a really fundamental point involved. In a certain sense (I am going to say something which will sound very unprecise) it means that the object and the subject cannot be separated. I have stated that too drastically and will illustrate what I mean. In classical physics it was taken for granted that the object observed and the act of observation could be completely separated. Now every physicist knew that the observation might disturb the

object; anyone knew that if you wanted to use a balance you could be clumsy and upset that balance while you were weighing, and that would affect the observation. But there didn't seem to be any essential reason why you couldn't be less clumsy. For example, your body temperature would set up air currents and therefore you put your balance under a glass housing and manipulated it from the outside. You knew, of course, that you could not see what the balance was going to show unless you had light to observe it, and you knew that if you took a klieg light to observe it you would heat your balance and measure wrongly. All that the classical physicist knew just as well as we, but there was no reason classically why you could not conceive a limiting process in which you decrease the disturbance of the process of observation. You could take a strong light, and then a light half as strong, and then a light quarter as strong, etc. Each of these lights would disturb the balance less and less and there was no reason to see why you could not extrapolate that process to an infinitely weak light which, presumably, would not disturb your balance at all. You could imagine such a series of measurements actually performed, and you then assumed that this final measurement (not really achieved but extrapolated) was what the balance would show by itself without your looking at it, and this you would call the true weight.

It was consistent, therefore, and logical, to talk about the true weight of the balance, or, if you prefer, the true position of the needle. This argument is now made impossible—not physically impossible, but fundamentally impossible by the fact that light consists of quanta, and there is no continuous transition from one quantum to zero quanta. Therefore either you do not observe at all, and then you can not know what the balance reads, or you have to use at least one quantum of light, and this one quantum is going to cause the minimum perturbation, but you do not know how much. Therefore your measurement by extrapolation can not be carried through because when you decrease you cannot decrease the amount of your light continuously. In the lowest steps you have fluctuations.

This simple argument is the essence of the situation. It means that the very fact that you observe, that you try to learn something about the object, will perturb that object to a minimum amount below which you cannot go due to the fundamental laws of the world around us. I will not go into details, but they work out in such a way that we cannot measure, and therefore cannot know exactly at the same time both the position and the velocity of a particle.

Now this uncertainty is, in absolute terms, the same for large and small bodies, but for large bodies it does not matter in practice, that is to say, for a chair or a drop of water this uncertainty is much

smaller than anything I can measure experimentally. However, for an elementary particle, e.g., for an electron or an atom, it is large enough to ruin my measurements. Therefore we are now confronted with a situation where even if you believe the statement of Laplace with which I started, nevertheless Laplace's condition is in itself unrealizable, not for reasons of lack of technical ability or technical development, but fundamentally. This means that I will never be able to make exact predictions about the future behavior of any particle. The future will at the beginning not be very much less certain than the original determination of my starting point, but the possible paths will diverge and hence after a sufficiently long period of time I will know nothing any more. The situation is such that if I had a beam of light rays that were really parallel then it would throw on the wall a light spot that would be just as big as the source from which it started out, but if the beam diverged then the spot made on the wall by the beam would be very much larger than the source. The further out I go, the broader the beam becomes, and the less I can tell about it. This has been translated into the fact that physics is really not deterministic, but that the only things that have value are statements about probabilities. What was for classical physics a matter of convenience, has now become a method of necessity.

There has been some discussion in the past whether that tells us anything about the problem which I have only touched. Bohr says that this situation, in the case of elementary particles, has led to the fact that one must use a dual description; i.e., if you ask me whether light consists of waves or particles, then I must answer that if you mean by waves something like water waves and if you mean by particles something like billiard balls, then it is neither, but it behaves in certain circumstances like the one and in other circumstances like the other. This is something which is now called complimentarity; it demands a radical change in our views and means that we simply cannot picture the real processes in terms of macroscopic images.

THE ORIGIN OF TERRESTRIAL MAGNETISM

Frank R. Haig, S.J.

The problem of the origin of terrestrial magnetism continues to plague modern physicists. Despite the tremendous endeavours of some of the greatest physical theorists, it remains unresolved to this day. It would seem profitable and interesting, therefore, to consider the chief theories that have engaged scientific attention as possible answers to this question.

The hypotheses so far advanced may be reduced to three classes

and labeled for convenience the ferromagnetic, the electromagnetic, and the gyromagnetic theories.

William Gilbert, court physician to Elizabeth I, is the most famous advocate of the ferromagnetic hypothesis. Having discovered that the magnetic field of the earth may be simulated by a magnetite sphere, his well-known *terella*, Gilbert postulated that the entire earth is composed of magnetite which, however, breaks down on the surface of the earth into the rocks of common experience.

The ferromagnetic theory, of course, would not be proposed in so naive a fashion today. Nippoldt (1), for instance, has suggested that the principal part of the earth's magnetic field consists of a non-homogeneous magnetisation of the earth's crust down to 20 km. He also speculated about a magnetic field for the earth's core and an interplanetary field arising ultimately from the sun.

Any ferromagnetic theory, however, faces seemingly insuperable difficulties. It leaves unexplained the origin of the magnetisation. It must postulate a very great retentivity in the magnetised material in order to maintain the magnetisation over such a long period as the earth's field is presumed to have lasted. But this very high retentivity runs counter to the known considerable diurnal, annual, and secular variation of the geomagnetic field. Under such conditions of constant change, the field apparently should have vanished long ago.

What is considered to be the gravest objection, however, is the fact that ferromagnetic substances become paramagnetic with sufficient heat and it would appear that the critical temperature for this change is reached at a mere 25 km. below the surface of the earth. It is true that some cosmologists have raised doubts about the assumed fairly high temperature of the earth's interior (2) but their work is still very hypothetical. In view of these difficulties the ferromagnetic hypothesis is not considered today.

The electromagnetic theory assumes an equatorially globe encircling electrical conduction current to explain terrestrial magnetism. The standard difficulty against this theory is the lack of an agent to maintain such a current.

Elsasser (3) developed the electromagnetic theory in such a way that the geomagnetic field becomes a rather accidental consequence of fluid motions taking place in the interior of the earth. The mechanism is rather elaborate and indirect. Elsasser assumes a small amount of radioactivity distributed through the earth's core. The heat produced from the radioactivity is carried away in the main by conduction. But the system is mechanically unstable and convection currents would arise carrying warm matter upwards and establishing variations in temperature. These variations in temperature give rise to a thermo-

electric force which drives the current producing the geomagnetic field. In order to insure that the thermoelectrically generated current will not vanish around the whole earth, the necessary asymmetry is explained by considering the Coriolis forces on vertically moving mass elements, by phase changes in the materials, and by sedimentation rates—on the whole, a rather complicated mechanism but perhaps demanded by the complexity of the effect.

Frenkel (4) developed a rather similar theory with the addition of a self-excitation mechanism. But Cowling's work (5) would seem to indicate that neither of these two theories give high enough values. Bullard (6), however, has used the electromagnetic theory rather successfully to explain secular variation. In summary, then, the electromagnetic theory, though not at present satisfactorily developed, is very much in the running as an explanation of terrestrial magnetism.

Perhaps the most interesting of the theories so far proposed to explain geomagnetism is the gyromagnetic theory. In accordance with this hypothesis, every massive rotating body has a magnetic field associated with it from the very fact of its rotation.

The only obvious atomic phenomenon that could give rise to such a field is the gyromagnetic effect, but, among other difficulties, the field so produced is far too small (7). If terrestrial magnetism is to be explained by the mere fact that the earth is rotating, this can only be done by finding some new law of physics connecting angular momentum and magnetic moment.

Wilson (8) first formulated such a principle. He noted that the magnetic fields of the earth and the sun could be explained by assuming that a moving mass element m, taken in gravitational units, has the same magnetic effect as a moving negative charge q measured in e.s.u. Formally, this would be expressed

$$H = -\frac{G^{1/2}\, m}{c r^3} [v \times r]$$

where H is the magnetic intensity, G the gravitational constant, and c the speed of light.

In this form the statement is obviously false, since it predicts magnetic fields from pure translational motions which are not observed. Let it be assumed, however, that v is restricted to velocities resulting from rotational motion. Then this immediate objection is removed and by proper manipulation the equation known by the work of Schuster, Wilson, and, more recently, Blackett (9), can be derived:

$$P = \beta \frac{G^{1/2}}{c} U$$

where P is magnetic moment, U angular momentum, and β presumably a universal constant of nature of the order of unity.

This equation is known to be approximately true for the earth, sun and several stars. It is in reality this equation which has sparked theoretical work on the gyromagnetic theory since the equation appears to enuntiate a new physical principle connecting gravitation and electromagnetism. The theory, however, has been rather roughly handled by the experimentalists.

At the meeting at which Blackett presented his first paper on this subject, Dr. Bullard pointed out that it should be possible to decide between a fundamental theory of geomagnetism which would hold that each element of the earth's mass contributes to the terrestrial field, and a core theory which would maintain that the field is largely produced in the presumed liquid core of the earth, by observing the variation with depth of the magnetic elements. H should increase with depth in a core theory but decrease in a fundamental theory.

Following Bullard's suggestion, after some preliminary confusion, Runcorn (10) seems to have experimentally disproven the fundamental or gyromagnetic theory. H apparently does increase with depth, as a core theory would predict. There is one way out for the proponents of the gyromagnetic theory and that is to suppose that the parameter β is not a universal constant but depends on the physical conditions of the body, differing from zero only in cases like the earth's core, the sun, and the stars. Luchak (11) has actually proposed this and pointed in justification to somewhat similar developments in the London and London theory of superconductivity.

Even should this seemingly *ad hoc* modification work, the gyromagnetic theory still leaves unexplained the fact that the magnetic axis and the rotational axis do not coincide nor does the theory explain secular variation.

To conclude: three theories have been seriously proposed to explain the origin of terrestrial magnetism, a ferromagnetic, an electromagnetic, and a gyromagnetic theory. The ferromagnetic is discounted today. The gyromagnetic has met serious difficulties. The electromagnetic is perhaps the best bet. In point of fact, however, we simply do not know the origin of terrestrial magnetism.

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Mathematics

BATTING AVERAGES

JOSEPH A. PERSICH, S.J.

Baseball fans often wonder what will happen to a favorite player's batting average if he hits or fails to hit safely in his next official time at bat.

The increase or decrease in his batting average depends on two things: the previous number of times at bat and the previous number of hits. If two batters have each been to bat the same number of times, the increase in their batting averages will not be the same if each hits safely in his next time at bat unless each had the same previous number of hits.

To find the increase or decrease in a batting average, use the formulas obtained by solving these two equations for x:

$$\frac{H}{AB} + x = \frac{H+1}{AB+1} \text{ if he hits safely;}$$

$$\frac{H}{AB} - x = \frac{H}{AB+1}, \text{ if he fails to hit.}$$

In these equations, $\frac{H}{AB}$ is the present batting average, x is the

increase or decrease in the batting average, $\dfrac{H + 1}{AB + 1}$ is the new batting average if he hits safely, and $\dfrac{H}{AB + 1}$ is the new batting average if he fails to hit.

Solving these equations for x, we obtain these formulas:

$$(1)\ \text{Increase} = \frac{AB - H}{AB(AB + 1)}$$

$$(2)\ \text{Decrease} = \frac{H}{AB(AB + 1)}$$

A batter has three hits in nine trips to the plate for an average of .333. If he hits safely in his next time at bat, the increase in his batting average will be $\dfrac{AB - H}{AB(AB + 1)}$, or $\dfrac{9 - 3}{9(9 + 1)}$, or .067. His new batting average is .400.

If he fails to hit safely, the decrease in his average will be $\dfrac{H}{AB(AB + 1)}$, or $\dfrac{3}{9(9 + 1)}$, or .033. His average would drop to .300.

If he had had two hits in nine official times at bat, a hit the next time up would have increased his average from .222 by $\dfrac{9 - 2}{9(9 + 1)}$, or .078 to .300. If he failed to hit, his average would have dropped .022 to .200.

By adding formulas (1) and (2) we obtain this formula which is sometimes useful:

$$(3)\quad \text{Increase} + \text{Decrease} = \frac{1}{AB + 1}.$$

In each case above the sum of the increase and the decrease is .100. Formula (3) may be used to find the increase if the decrease is already known.

In the closing stages of a pennant race the change in a regular's batting average is small. Rounding off the numbers in the fraction

to the nearest or a near tens' or hundreds' digit will give a quick and accurate approximation of the change in the player's average.

In 1952 Stan Musial won the National League batting crown with an average of .336 based on 194 hits in 578 times at bat. If he had gone to bat one more time and had hit safely, the increase in his average would have been $\dfrac{578 - 194}{578(579)}$, or approximately .00115 or slightly more than a point.

If he had gone to bat that extra time and had failed to hit safely, his average would have dropped $\dfrac{194}{578(579)}$, or about .00057 or slightly more than ½ a point.

The sum of the increase and the decrease would be $\dfrac{1}{579}$ or about .00173.

What happens to a player's batting average if he gets two hits in three times at bat or if he goes hitless in five trips to the plate?

Letting H and AB represent the present number of hits and times at bat, and NH and NAB the additional hits and additional times at bat, we have these two fractions:

$\dfrac{H}{AB}$ is the current batting average, and $\dfrac{NH}{NAB}$ the ratio of the additional hits to the additional times at bat.

To find the increase or the decrease in the player's average and cover all contingencies, first set up these two fractions in this order: $\dfrac{H}{AB}, \dfrac{NH}{NAB}$. Considering AB and NH as the means, and H and NAB as the extremes, it can be shown that the change in the batting average is given by the fraction whose numerator is the product of the means — the product of the extremes, and whose denominator is AB(AB + NAB). The fraction is written in this way:

$$(4) \quad \frac{NH(AB) - H(NAB)}{AB(AB + NAB)} = \text{the change in the batting average.}$$

If the numerator is positive, the average increases; if it is negative, the average decreases. We obtain this formula by solving the equation

$$\frac{H}{AB} + x = \frac{H + NH}{AB(AB + NAB)}$$ for x, where x represents the change in the batting average.

Formula (1) is a special application of Formula (4) in that NH and NAB are each equal to 1. Formula (2) is a special application of Formula (4) in that NH is equal to 0, thereby causing the first term of the numerator to drop out, and NAB is equal to 1.

A few examples will show the application of Formula (4). A batter has 4 hits in 16 times at bat for a .250 average. He gets three hits in four trips. The fractions are $\frac{4}{16}$, $\frac{3}{4}$. The change is $\frac{3(16) - 4(4)}{16(16 + 4)}$, or $\frac{32}{16(20)}$, or $\frac{1}{10}$ or $+ .100$. His new average is .350.

Another batter has 5 for 8 for .625. He gets one for two. The fractions are $\frac{5}{8}$, $\frac{1}{2}$. The change is $\frac{1(8) - 5(2)}{8(8 + 2)}$, or $\frac{-2}{80}$, or $-.025$. His new batting average is .600.

If a player fails to get an additional hit, the first term of the numerator becomes 0. The change in his batting average will be $\frac{-H(NAB)}{AB(AB + NAB)}$. If he had 9 for 27 for a .333 average, and then went hitless in his next three trips to the plate, the change would be $\frac{-9(3)}{27(27 + 3)}$, or $-.033$. The new average would be .300.

Simple examples have been chosen in this article to better illustrate the principles. Persons skilled in quick mental arithmetic should be able to apply these principles in the more complicated cases that arise.

Applications of these formulas in other fields are left to the reader.

SOVIET SCIENCE

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[Note: This article is based on a symposium presented on December 27, 1951, at the Philadelphia meeting of the American Association for the Advancement of Science. The various papers given at the symposium were collected and published in book form by the AAAS under the title, "Soviet Science" (1952).]

A great deal has been written about Russia in general, and relatively little about Russian science in particular, during these days. What of the heritage of Russia's scientific giants: Mendeleev in chemistry; Mechnickov, Vinogradskii, and Omelyanskii, the biologists; Chebichev and Lobachevskii in mathematics; Pavlov, the methodologist par excellence in physiology? They are not alone; many more illustrious names could be added to this scanty list. By any scientific standard the work of these scientists would excel.

That the Bolshevik regime has effected modern Russian science and Russian scientists there is no doubt. This course of Soviet scientific progress can be rather accurately charted since we do know what is happening in the research laboratories behind the Iron Curtain. Our knowledge of present-day Soviet science is restricted less by Russian intervention than by relatively small printings, poor distribution facilities, and the inability of many scientists to read Russian.

History

Peter the Great by building the Kunstkamera, a science museum of chemical and physical apparatus, was the first of the Tsars to emphasize science in Russia.[1] His projected Imperial Academy of Sciences was constructed by Catherine I, his successor, in 1725. German, French, and Swiss scientists were introduced to the institution; of these the German influence was the strongest. It was natural that after a time a reaction to the foreigners should develop. Michael Lomononosov, the son of a peasant, led the revolt, and founded the University of Moscow in 1755 after having been trained in European schools.

However, Russian research, save for the work on electrical phenomena by Jacobi and Lenz, made slight impression on the world from about 1750 to 1850. Bringing to light the work of this era is part of the attempt of the governmental program to increase national pride.

[1] The monarch fancied himself to be a doctor of medicine and used to forcefully operate on people, very often with fatal results.

According to the Russians marvelous discoveries, unheard-of outside Imperial Russia, were made in this age of scientific enlightenment (e.g. the steam engine of Palzunov, the incandescent lamp of Lodigin, and Popov's radio). Actually, these men had little effect on their followers.[2] It was the scientists of Western Europe and the excellent training given at the two science centers of St. Petersburg and Moscow that fathered the Russian science of today.

"The priest, the doctor, and scientist are slaves of the capitalists". Such was the decree of the Communist Manifesto. The scientists who were tolerated but not encouraged under the Tsars, were to be made the slaves of the proletariat. At the time of the Revolution many prominent scientists left Russia, and those who remained found themselves opposed to rigid Communist doctrines. The Soviets tried to create a new class of scientists from the workers and peasants, but the project was abandoned as a failure just before World War II. The status of the scientist has rapidly evolved from the slave of the workers in the three-class society of worker, peasant, and intellectual, to that of the highest, the intellectual. As members of the intelligentsia and upper middle class top-flight scientists are respected and well-paid. A prime example of the present respect of science and scientists in Russia is the fact that the President of the USSR Academy of Sciences possesses the rank equal to that of a major minister in the government.

The number of publications is a good guage of the scientific activity of a country—the USSR Academy of Sciences publishes extensively. Dodlaki (Reports) of the Academy is the major publication. It comes out 36 times a year and covers all branches of science. In 1947 the policy of carrying a German or English title accompanying the Russian article, was dropped. Also, the publishing of foreign language periodicals was discontinued. Coupled with this difficulty of non-Russians not being able to find out what was happening in Russian science was that of decreasing personal contacts between Russian and foreign scientists.[3] Despite the lack of zeal for personal contacts, the Russians follow very closely the progress of Western science and publish very good articles of this progress.

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[2] Butlerov and Mendeleev were exceptions exerting a great influence in chemistry (Mendeleev was a good physicist also).

[3] A somewhat amusing example of this chauvinism in Russian scientists relations with foreign scientists is an incident that happened at a science celebration in Warsaw in 1948. The common tongue used was French but the two Russians present insisted on speaking Russian despite the fact that they spoke French fluently and the Russian they spoke had to be translated into French.

Physics

In general it can be said that research in Soviet physics is extensive, and in many fields, excellent. Definite schools or research are conducted in: cosmic rays, nuclear phenomena, theoretical physics, low-temperature investigations, solid state physics, electron optics, and luminescence. For years the Soviet has been well represented in theoretical physics by men like V. A. Fock (quantum mechanical calculation of the structure of complex atoms), D. D. Ivanenko (nuclear structure), and Lifhits (low-temperature phenomena). In the past few years Stalin prizes were awarded to men for their outstanding work in: "Theoretical Hydrodynamics" (Lavrentiev), "Vibrations of Molecules" (M. V. Vol'kenstein), and "The Theory of the Origin of Cosmic Rays" (Ya. P. Terletski).

Strong research centers in the various branches of physics exist throughout the USSR. At Moscow there is the Kapitsa laboratory (The Institute of Physical Problems) where experimentation in low-temperature phenomena is carried on. A. Joffe, an eminent physicist who studied with Roentgen in Munich and who wrote on the mechanical and electrical properties of crystals, founded the Physico-Technical Institute at Leningrad to investigate solid state physics. The Lebedev Physical Institute in Moscow was named for P. N. Lebedev, a great researcher in optics. This institute was directed by S. I. Vavilov,[4] a student of Lebedev, until his death in 1951.

Chemistry

Research in chemistry has been more extensive though less notable and less theoretical than in physics. Since the Bolsheviks came to power in 1917 there has been a major attempt made to develop heavy chemical industry so that the country might be independent of foreign chemical imports. Because of this emphasis there have been a great many papers written on specific minor chemical problems.

The various branches of chemistry are being developed in many laboratories: inorganic chemistry at the Kurnakov Institute of Inorganic and General Chemistry and at the Khlopin Radium Institute[5] (founded by V. G. Khlopin in 1919); Thio-Organic Chemistry Institute with the president of the Academy, A. Nesmeyanov, as director; the Physical Chemistry Institute and the Chemical Physics Institute, the latter headed by the famous physical chemist, N. N. Semenov; and in the analytical field, the Vernadski Institute of Geochemistry and Analytical Chemistry. Specific fields of investigation

[4] Vavilov was the editor of the Grand Soviet Encyclopedia which is to soon appear in 50 volumes.

[5] The first cyclotron in Europe was built here in 1937.

worthy of mention are: the development of organo-metallic compounds as anti-knock agents for internal combustion engines, phosphorous acids, chemical kinetics, and catalysis. The research value in catalysis was early recognized by the Communists with the result that much catalytic has been done in syntheses of motor fuels and synthetic rubber.

Physiology and Pathology

The father of Russian physiology was I. V. Sechenov (1825-1905) whose work on conditioned reflexes greatly helped his illustrious successor, Pavlov. Pavlov received the first Nobel Prize awarded in medicine and achieved the greatest international fame of all Russian scientists. In his lifetime he performed very fruitful experiments on the circulatory, the digestive, and the nervous systems of the body, exhaustively studying the conditional reflex. Bykov, Anokhin, Kupalov, and others carried on the great work of Pavlov, but the next real luminary in the field was A. D. Speransky. Speransky proposed that the production of disease is primarily inherent in the nervous system. Another tenet of his was that medicine should rely more on its own principles, instead of seeking an outside discipline.

The study of conditional reflexes is proper to Russia and the Pavlovian school. Russia takes great pride in this fact, so much so that in 1950 there was a joint session of the Academy of Sciences of the USSR and the Academy of Medical Sciences of the USSR[6] dedicated to studying the work of the great man. One of the results of the conference was a three column list of suggestions appearing in Pravda. Lena Stern, I. S. Beritov, Orbeli,[7] and other prominent physiologists were criticized for not following the nationalistic traditions set down by Pavlov and Michurin and for being influenced by Western science. Western science was criticized also, with the exception of Darwin. Freud's psychonalysis was condemned as "a fragment of bourgeois democracy". The fact of the matter is, the Russians are still too occupied with basic problems of epidemics, nutrition, and surgery to be engaged in psychoanalysis to any extent.

Psychosomatic medicine was next in the line-up of offenders and had its hand slapped like the others, because it implies a dualistic nature of body and mind, the mind being superior to the body. We would greatly err, however, if we ascribed all the reasons of the Russians

[6] No American or English scientists were invited to this conference.

[7] A. Zhdanov who wrote the article, thus attacked Orbeli: "The point is that Academician Orbeli's scientific interests and work methods were formulated not so much under Pavlov's influence as under the influence of Western European physiologists with their metaphysical and idealist concepts". "Scientific Sessions on the Teaching of Pavlov". Foreign Language Publishing House, Moscow, 1951.

for rejecting many Western methods to fierce Communistic chauvinism. Many of the objections they give to practices current in the West are valid (e.g. against lobotomy they argue that the man's personality is permanently changed). Most of the biological science and theoretical medicine is not immediately applicable to therapy, or is in the experimental stage and of questionable value. Consequently the rigid form of Russian science and its necessary link with politics and enforced classical lines of research will not be actually felt for some time to come. It is interesting to note the paradox of politically-minded Soviet biologists who must dogmatically follow Pavlov, a man that spoke his own mind freely. In spite of the fact that he once said that he would not sacrifice "a frog's hind leg for the social experiment in Russia", he was, nevertheless, a rabid patriot and is recognized as such by Russia today.

Genetics

During the twenties and thirties so much research was conducted in this science that Russia was second only to the United States in the amount of work done. N. I. Vavilov, the brother of the President of the Academy, was the leading figure in genetics. From 1935 on genetics and geneticists were attacked by a group under the leadership of T. D. Lysenko who proposed the "Michurinist Doctrine". The school went back to the nineteenth century theories of Michurin and others, which were discarded in the light of discoveries made in genetics.

In the thirties and forties Lysenko was a Darwinist, but he changed to Lamarckism in recent years. The hold of Michurinism with its Lamarckian interpretation was strengthened when in 1948 Lysenko discovered that there can be no competition or struggle between members of the same species. Since the purge of the geneticists of whom N. I. Vavilov was the most notable, and the government's support of Lysenko and his theory the work is, for the most part, stereotyped though some original work is going on.[8] Many geneticists are doing research on problems only remotely connected with basic genetics in order to avoid possible censure of conflicting theories.

(To be continued)

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[8] Lysenko claims that he is able to change wheat into rye, regarded as an almost impossible feat by geneticists at large. These results of Lysenko's have not been reproduced outside of the USSR.